Is Retrospection Blind?
Random Events, Economic Losses, and Voting Behavior

(Research Note)

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ABSTRACT

Recent research that has studied retrospective voting in non-economic domains has concluded that voters irrationally blame politicians for events, such as natural disasters, that are outside of the control of elected officials. However, these studies cannot disentangle the events themselves from how voters expect government to prepare for and respond to those circumstances. We address this limitation by leveraging two natural experiments in which we are able to tie exogenous, natural events to economic outcomes, for which politicians could reasonably be held accountable. First, we find that voters punish incumbents for tornado-induced economic damages, but not for tornado-caused fatalities. Second, we find that abnormal rainfall hurts incumbents only to the extent that farm income is affected. Voter behavior is consistent with rational decision making, since people may either blame the government for inadequate preparation/response or face information constraints that make it impossible to decompose overall economic conditions into components for which the government is and is not responsible.

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The extensive literature on retrospective voting demonstrates that voters reelect incumbents who have been stewards of peace and prosperity, and remove those who have presided over bad times (e.g., Kramer 1971; Fiorina 1981). Most of this early work and much of the subsequent research has focused on how macroeconomic conditions affect election outcomes in this way (e.g., Powell and Whitten 1993; Markus 1988; Lewis-Beck and Stegmaier 2007). Scholars have typically viewed such behavior as evidence of “a responsible electorate” (Key 1966) using diagnostic information to make reasoned judgments about the effectiveness of elected officials.

A recent line of research has challenged the consensus that voter responses to the state of the economy reflect the behavior of responsive citizens holding elected officials accountable for their actions. These studies aim to show that voters reward and punish elected officials in response to circumstances beyond any incumbent’s control, and are therefore behaving irrationally. For example, Wolfers (2009) finds that the reelection prospects of U.S. governors of oil-producing states are affected by changes in the international price of oil, a variable beyond a governor’s direct control. Similarly, Leigh (2009) shows that voters reward national leaders who are in power when the world economy booms, regardless of their true competence as economic managers as measured by their own country’s growth rate relative to the global growth rate. Finally, Achen and Bartels (2004a) find that “voters regularly punish governments for acts of God, including droughts, floods, and shark attacks” (2).

However, in all of these examples, incumbent politicians may be expected to prepare for or respond to the events that are outside their direct control. The government can pursue policies aimed at diversifying the state economy, institute trade policies to change how a country interacts with the global economy, invest in preparing for natural disasters, provide aid to those adversely affected by floods and droughts, and assist the tourism-related businesses that lost customers due to shark attacks.[1] Consequently, observing that incumbents are adversely affected by droughts and floods, for example, may not necessarily mean that voters are irrational. Even though government cannot be blamed for the adverse natural events themselves, they can be held responsible for mitigation, response, and recovery. Moreover, previous evidence could be reconciled with a model of rational voters facing information constraints. Voters may not possess the information needed to accurately assess the extent to which government efforts and policies did or did not matter with regards to their welfare. Information-constrained but otherwise rational voters may then consider the total changes they observe in their own and others' standards of living as they make decisions if it is either not possible or too cognitively demanding to decompose income changes into the pieces coming from external forces, such as the global price of oil, and other factors plausibly within the incumbent's control.

In this paper, we find that voters indeed do appear to respond to events beyond any incumbent's control. We add to the existing literature by demonstrating that voters respond only to the economic damages associated with the events—for which government could reasonably be held responsible—but not to other kinds of losses such as fatalities associated with those events or other non-economic costs. Hence, voter responses to these events appear at least to be consistent with traditional conceptions of retrospective voters processing diagnostic information to hold government accountable for its actions, as opposed to being clear evidence of arbitrary, myopic, or blind retrospection (Achen and Bartels 2004a, 2004b).

To demonstrate this, we leverage two natural experiments. First, we assess the effects of economic damages and fatalities induced by tornado occurrence on the incumbent presidential

[1] According to Achen and Bartels (2004a, 12), the economic losses in New Jersey due to the shark attacks were substantial. "Some resorts had 75 percent vacancy rates in the midst of their high season (Capuzzo 2001, 274). Losses may have amounted to perhaps as much as $1 million for the season altogether, a sizable sum in 1918."

party's vote share in a county. We find that monetary losses due to tornadoes reduce incumbent vote share, but that tornado-related fatalities have *no* effect. We interpret this pattern of results to suggest that tornadoes may not harm incumbents' reelection prospects because citizens irrationally blame the government for the natural events themselves, but because they have been economically affected and therefore demand government action. Second, like Achen and Bartels (2004a), we examine how droughts and floods caused by extreme rainfall influence voting behavior. We find that the effect of rainfall on voting varies according to a state's economic dependence on agriculture, again demonstrating that it appears primarily to be the economic consequences of the disaster that affect voting behavior, as opposed to emotional or affective responses to the event itself.[2]

This note is organized as follows. We first present an overview of our natural experiments, explaining the advantages of our research design over studies that have explored macroeconomic outcomes. We then present our analysis of the effects of tornado damage and fatalities, describing the method, data, and results. Next, we examine the effects of abnormal rainfall patterns. We conclude by summarizing our findings and providing directions for future research.

Two Natural Experiments: Tornadoes and Abnormal Rainfall

Despite the literature's primary focus on the macroeconomy, economic conditions are non-ideal tests to assess the impact of negative outcomes and events on election results (Alesina, Londregan, and Rosenthal 1993). First, indicators such as GDP growth and the unemployment

[2] While both Achen and Bartels (2004a) and we observe a relationship between abnormal rainfall and incumbent vote share, Gomez, Hansford, and Krause (2007) find that Republican politicians do better when there is rain on Election Day through the mechanism of differential turnout. This effect is not necessarily evidence of irrationality, since Republican voters may experience a different cost of voting than do Democratic voters.

rate are difficult to pin down temporally. Assessing when the economy truly begins to contract and materially affect citizens is difficult for voters (and researchers) to determine. Second, economic outcomes are not randomly assigned, either across time or space. For instance, incumbents who are especially politically skilled may also be good managers of the economy. Any temporal correlation between economic conditions and election outcomes may be due to this omitted variable. Additionally, election outcomes may affect economic conditions as opposed to causation running in the opposite direction.[3] Finally, it is difficult to categorize commonly-used economic indicators as unambiguously "good" or "bad" given that they have heterogeneous distributional consequences. Whereas inflation tends to adversely affect higher-income individuals, unemployment is mainly shouldered by those in the middle and lower classes.

To obviate these issues, we explore the effects of two exogenous natural phenomena on election outcomes: tornadoes and abnormal rainfall patterns. Tornadoes in particular, and rainfall to some extent, can be precisely observed in small geographic areas, thereby reducing measurement error. In addition, since tornado occurrence and weather patterns are, conditional on a county's *ex-ante* exposure, randomly assigned, it is plausible to assume that causality runs in the direction of negative outcomes to election results, and not vice versa.

Study One: The Effects of Tornado Damage on Presidential Elections

Data and Empirical Strategy

In this section, we combine county-level data on voting and tornado damage to explore the ways in which incumbents are punished for natural disasters. In other words, do voters

[3] For example, officeholders may direct more resources to supportive areas or areas that are becoming more supportive (Chen 2008). Or, voters may become more optimistic when their favored candidates win, thus increasing demand (Gerber and Huber 2009).

narrowly punish politicians for economic damages stemming from tornadoes, or do they more broadly (and perhaps irrationally) blame them for tornado-related fatalities? To measure tornado damage and fatalities, we use the county-level records collected by the National Climatic Data Center (NCDC) (2008) since 1950. This database consists of all tornado occurrences recognized by the National Weather Service. It provides information on the county of occurrence for 1988 and earlier. For 1989-2004, we determine the county of occurrence based on the town in which the tornado is reported to have struck. We are able to match the tornado occurrences to counties for 96% of the tornado occurrences from 1989 to 2004. We combine the tornado data with the county-level presidential election returns collected by Clubb, Flanigan, and Zingale (2006), supplementing these data with Congressional Quarterly's (2006) data for 1992-2004. For county population, which is used to create the estimates of per-capita tornado damage and fatalities, we use the Census estimates of county-level population and interpolate those amounts to create estimates for the intercensal years.

For estimation, we consider the following general model of voting behavior:

Incumbent Vote Share = f (Party's Previous Vote Share, Tornado Damage, Tornado Fatalities, Economic Conditions, Demographic Variables)

We also include county and year fixed effects in our regressions to isolate the impact of within-county variation in tornado damages and fatalities. The inclusion of county fixed effects controls for a county's average exposure to tornado damage, ensuring that the coefficients of interest are identified by temporal variation in damage from that baseline.

To address the large amount of skewness in tornado damage and fatalities, we follow other authors (e.g., Ansolabehere, Gerber, and Snyder 2002) and convert these measures into logarithms. For example, the damage measure that we use is the logarithm of per-capita damage plus one. The one is added so that, when we take the logarithm, the measure is mapped back to zero for the case of zero damage. In equation form, where *TotDam* is total tornado damage in inflation-adjusted January 2008 dollars and *Population* is the county's population, we use the following measure of tornado damage:

$$Damage = \ln\left(\frac{TotDam}{Population} + 1\right) \quad (1)$$

We operationalize tornado fatalities similarly, also taking the log. We obtain similar results both in terms of statistical significance and substantive meaning when using unlogged measures, but we regard our results obtained using logged damage and fatalities as more relevant since they are less sensitive to outliers.

Results

Although there is a significant correlation between damage and fatalities, the correlation between the two is sufficiently small so that it is possible to investigate the separate effects that damage and fatalities have on election outcomes. Only 20% (11 of 54) of the cases in which there were at least five fatalities were also among the 54 highest instances of per-capita damage. 27% (76 of 277) of the cases in which there was at least one fatality were also among the 277 highest instances of per-capita damage. Overall, the correlation coefficient between the log of per-capita damage and the log of per-capita deaths is .21. The correlation coefficient between the log of per-capita damage and the absolute number of deaths is .26. These relatively weak correlations arise primarily because tornadoes of intermediate strength are correlated with damage nearly as strongly as the strongest tornadoes, while only the strongest tornadoes are correlated with deaths. The correlation coefficient between a county's log per-capita damage and the number of tornadoes it experienced in the two most severe categories (F4 and F5) is .37, while the corresponding correlation coefficient between the log of per-capita fatalities and the number of F4 and F5 tornadoes is .34. On the other hand, the correlation coefficient between log per-capita damage and the number of F3 tornadoes is .27, while the correlation coefficient between the log of per-capita fatalities and the number of F3 tornadoes is only .10.[4] In the data, F3 tornadoes occur about 2.5 times more often than tornadoes of greater strength.

Simple summary statistics suggest that economic damages due to tornadoes reduce the incumbent presidential party's vote share, but that tornado-related fatalities do not have a commensurate effect on voting behavior. In Table 1, we consider the average change in incumbent party vote share for cases of high amounts of tornado damage and cases of high numbers of tornado-caused fatalities. The first two columns refer to the change (from the previous election) in the incumbent party's vote share in the affected counties and the average change in the incumbent party's vote share nationally for those observations. For example, the -3.47 in the first row and first column indicates that the average change in incumbent vote share for the worst 50 county-year observations of tornado damage was -3.47%. The -1.45 in the second column indicates that, for these 50 observations, the incumbent party's vote share fell nationally by an average of 1.45%.[5] In other words, the incumbent party did, on average, 2.02 percentage points worse in the 50 highest-damage counties than it did nationally in those years, in terms of change in vote percentage. This difference is significant at the 10% level ($p = .09$). Expanding the sample to consider the highest 500 cases of per-capita damage, we find that the change in the incumbent party's vote percentage is 1.11 percentage points lower in the affected counties than nationally, a difference that is significant at the 1% level ($p = .01$).

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[4] The correlation between the log of per-capita damage and the total number of weaker tornadoes (F1 and F2) is .15, while the correlation between deaths and the number of F1 and F2 tornadoes is only .02.

[5] The fact that the average change is negative reflects regression to the mean. By virtue of winning the previous election, the incumbent party did well in that election and does, on average across a series of elections, a little worse in the current election.

Whereas these simple comparisons suggest that tornado damage hurts the incumbent party, no similar effect is apparent for fatalities. If we take the 54 cases in which at least five people in the county died due to tornadoes, the incumbent party did approximately .60 percentage points worse in the affected counties than it did nationally, an insignificant difference ($p = .72$) both statistically and substantively. Since some of these cases also had high monetary damage, these changes do not suggest that people vote against the incumbent party in response to fatalities *per se*.[6] If we consider the 277 cases in which there is at least one death in the county in the election year, we similarly find no effect of fatalities on election results. The incumbent party does .12 percentage points better in counties that experienced at least one death than it did nationally, an insignificant difference ($p = .85$).

These simple differences are suggestive support for the hypothesis that incumbents suffer electoral losses due to tornado damage, but that there are no losses associated with tornado deaths. Still, other hypotheses could also explain this apparent bivariate relationship. To provide further evidence, we consider a multivariate, fixed effects regression model.

We first predict incumbent party vote share with log per-capita tornado damage, county dummies, year dummies, and the incumbent party's previous vote share as independent variables (see Table 2). In the second column, log per-capita tornado fatalities is also included as a regressor. In the third column, we include the total number of fatalities instead. In the fourth through sixth columns, we add a variety of control variables, including income, unemployment, and the percent black in the county.[7]

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[6] Note that this finding contrasts with the effects found by Grose and Oppenheimer (2007) and Karol and Miguel (2007), who find that incumbents were adversely affected by Iraq war deaths. However, in the case of the Iraq war, the military deaths can be sensibly attributed to the policies implemented by both the executive and legislative branches.

[7] The control variables come from Gomez, Hansford, and Krause's (2007) dataset. Details on the sources of these variables can be found in their paper. We thank Tom Hansford for sharing the data with us.

The results show that tornado damage has a negative effect on the incumbent's vote share, whereas fatalities have no detectable effect. Damage is significant at the 10% level in all six columns. To interpret the coefficient estimates (around -1.5 across all specifications) in substantive terms, consider the implied cost of tornado damage in terms of the number of votes that the incumbent party loses. The regression estimate suggests that a 1% increase in per-capita tornado damage (an increase of .01 in the log) causes the incumbent party to lose approximately .015 percentage points of the vote. Although damage appears to hurt the incumbent party, the results suggest that fatalities have no such effect. The point estimates are close to zero in all cases. Whether we consider the log of per-capita fatalities or the absolute number of fatalities, we cannot reject the hypothesis that fatalities have no effect on voting decisions (p = .92 and p = .61 in columns (5) and (6), respectively).

What are the implications of these results for our understanding of retrospective voting? Consistent with Achen and Bartels (2004a), we find that incumbents appear to be adversely affected by natural disasters that they did not cause. However, we do not interpret these findings as necessarily being evidence of voter irrationality because of the heterogeneous effects of economic damage and fatalities. Voters punish incumbents for material damages wrought by tornadoes, perhaps because they believed that the government did not do enough to prepare for the storms *ex-ante*, or because they were dissatisfied with how the government responded to the disasters *ex-post*. Conversely, voters do not blame incumbents for deaths caused by the tornadoes, which are more random and less likely to be redressed by government response. Hence, our evidence is more consistent with voters reacting to the economic consequences of disaster damage, rather than irrationally blaming the incumbent for the event itself and any associated loss of life.

Study Two: The Effect of Floods and Droughts on Presidential Elections

Data and Empirical Strategy

To determine the effect that droughts and floods have on presidential elections, we utilize the NCDC's measurements of the Palmer Hydrological Drought Index (PHDI).[8] This index estimates the degree of moisture in the soil associated with recent weather conditions. If the PHDI is zero, recent weather conditions are consistent with a balance between moisture supply and the soil's demand for water. Hot and dry conditions will lead to negative PHDI values (with -4.00 and lower being considered "extreme drought"), while cold and wet conditions lead to positive PHDI values (with 4.00 and higher being considered "extreme wetness"). Extreme values of the PHDI indicate conditions that are unfavorable for agriculture. We combine this measure of the abnormality of election-year rainfall with state-level election returns from 1896 to 2004. We use basically the same dataset as Achen and Bartels (2004a), thereby allowing us to conduct both a replication and an extension.

To test the hypothesis that voters' decisions systematically depend on the weather, we regress the incumbent party's vote share in a state on the weather in the election year and a set of controls, including a full set of state and year fixed effects. The presence of the state and year effects means that the estimated effect of weather on voting comes from variation across states in the weather in any given election year.

Similar to Achen and Bartels (2004a), we measure weather conditions by the absolute value of the average of the PHDI between May and October in state s in year y. High values of this measure thus correspond to extreme weather in the election year. Using this measure as the

[8] The NCDC (1994) data was accessed in June 2006. The state-level election data for 1896 to 1988 comes from the Inter-university Consortium for Political and Social Research (1995). The state-level election data for 1992 to 2004 comes from Leip (2006). Returns from Alaska, Hawaii, and the District of Columbia are not included in the analysis.

independent variable of interest, we consider the following simple model of the incumbent presidential party's vote share:

Incumbent Vote Share = f (Party's Previous Vote Share, Abnormal Rainfall, Economic Conditions, Demographic Variables)

Results

The results indicate that droughts and floods have a negative effect on the vote share that the incumbent receives, but only in the states that are most involved in farming. In the American National Election Studies data series, 80% of American farmers have lived in three of regions as defined by the ICPSR: East North Central, West North Central, and the Solid South. Only in these twenty-two states, where weather patterns have had the greatest impact on income, does abnormal rainfall have a significant effect on voting behavior.[9]

The point estimate in the first column of Table 3 indicates that an increase of one in the drought measure has historically cost the incumbent about .058 percentage points, with the coefficient not being significant at conventional levels ($p = .18$).[10] At the same time, the coefficients in the second column show that, in the states in the three regions listed above, the incumbent presidential party experiences substantial losses when soil conditions deteriorate. The estimated effect for these farm states of -.139 percentage points, obtained by summing the

[9] The East North Central states are Indiana, Illinois, Michigan, Ohio, and Wisconsin. The West North Central states are Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. The Solid South states are Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, and Virginia.

[10] Our point estimate is very close to the -.062 that Achen and Bartels (2004a) report using the same PHDI measure and a similar regression specification. Whereas they report a *t*-statistic of 2.7, our *t*-statistic is only 1.4. This difference appears to arise due to the fact that we correct our standard errors for clustering at the level of the election. If we do not cluster correct our standard errors, we obtain a *t*-statistic of 2.2. If we also follow Achen and Bartels (2004a) and leave state fixed effects out of the regression, the *t*-statistic climbs to 2.8. We believe these reasons explain the differences between our results and those reported by Achen and Bartels. Our conclusion is that there is likely an effect of the drought index on election results at the state level nationwide, but that this effect is somewhat sensitive to the statistical specification.

coefficient for the linear term and the coefficient for the interaction term, is significant ($p = .01$). Moreover, the coefficient estimate on the interaction term itself is statistically significant, meaning that, compared to other areas, regions that are more dependent on agriculture exhibit a stronger relationship between rainfall patterns and voting behavior. The main effect is approximately zero, indicating that rainfall has no effect on voting behavior outside the set of farm states.

In column (3), we report the results we obtain by using the average share of farm income in a state's total income from 1929-2004 to estimate the importance of farming in a state. The interaction term here has the expected positive sign, although it is not significant at conventional levels. Importantly, the main effect for the flood/drought measure has a point estimate that is almost equal to zero, suggesting that in a state where farm had a zero share in income, the effect of abnormal rainfall on voting would likewise be zero. This result indicates that any aggregate effect of droughts and floods on elections is accounted for by the effect of extreme PHDI on income.

To summarize, we have found that the incumbent party appears to be punished for droughts and floods, but only in those states where farming has historically been important. In other words, the link between natural events and voting behavior appears to be via economic voting, and not an adverse reaction to the event itself.

Discussion

The study of retrospective voting has witnessed exciting advances in recent years, as scholars have begun to explore domains of government performance beyond management of the macroeconomy. These studies have leveraged the temporal and spatial isolation of non-economic outcomes in order to more precisely analyze government accountability. Many of these analyses have concluded that voters irrationally blame politicians for events, such as natural disasters, that are outside of their control. However, these studies cannot disentangle the events themselves from how voters expect government to prepare for and respond to the events. Hence, associations between the natural and political world are not necessarily evidence of voter irrationality.

This paper has attempted to address this limitation in previous work by tying exogenous, natural events to economic outcomes, for which politicians could reasonably be expected to be held accountable. We contrast the voter response to random events that lead to economic damages with the voter response to random events that cause non-economic losses. We found that voters punish incumbents for tornado-induced economic damages, but not for tornado-caused fatalities. Moreover, we find that any negative impact that abnormal rainfall has on incumbents is driven entirely by how dependent a region's economy is on agriculture. Indeed, in agriculture-dependent states, elected officials could be expected to deal with natural events that may adversely impact the local economy. In sum, the relationship between natural disasters and voting behavior does not appear to be clear evidence against reasoned retrospective voting. The fact that voters respond only to the economic losses associated with natural disasters suggests two plausible explanations for voter behavior that can be reconciled with rational voting. Voters may either be displeased with government preparation for or response to those natural events. Or, facing information constraints about the different causes of overall economic conditions, they follow a heuristic of voting against incumbents in times of economic hardship.

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Table 1:
The Effect of Tornado Damage and Fatalities on Presidential Voting, 1952-2004

	Average change in the county's vote	Average change in national vote (same set of years)	Difference
Tornado damage			
50 largest amounts of per-capita damage	-3.47	-1.45	-2.02* (p = .09)
500 largest amounts of per-capita damage	-4.07	-2.96	-1.11*** (p = .01)
Tornado fatalities			
All cases with at least five deaths	-3.70	-3.10	-.60 (p = .72)
All cases with at least one death	-4.10	-4.22	.22 (p = .85)

Note: All the damage and fatalities amounts refer to the year preceding a presidential election. p-values are for the test of equality between the change in the incumbent's vote share in the county and the change in the incumbent's national vote share. * $p<.10$, ** $p<.05$, *** $p<.01$.

Table 2:
Regression Results for Tornadoes

Dependent variable: Incumbent party's presidential vote share in the county

	(1)	(2)	(3)	(4)	(5)	(6)
Tornado damage measure	-1.528*	-1.535*	-1.475*	-1.516*	-1.503*	-1.453*
	(.789)	(.81)	(.802)	(.788)	(.809)	(.801)
Tornado fatalities measure		75.401			-129.249	
		(1265.164)			(1269.401)	
Number of tornado fatalities			-.031			-.036
			(.068)			(.07)
Incumbent's previous vote share	.589***	.589***	.589***	.568***	.568***	.568***
	(.038)	(.038)	(.038)	(.04)	(.04)	(.04)
Percent black				.072**	.072**	.072**
				(.03)	(.03)	(.03)
Percent high school graduates				-.117	-.117	-.117
				(.33)	(.33)	(.33)
Farms per capita				-18.544**	-18.541**	-18.534**
				(7.78)	(7.78)	(7.78)
Unemployment rate				-.054	-.054	-.054
				(.07)	(.07)	(.07)
Per-capita income				.208	.208	.208
				(.51)	(.51)	(.51)
Statewide share for incumbent party	.726***	.726***	.726***	.777***	.777***	.777***
	(.073)	(.073)	(.073)	(.073)	(.073)	(.073)
Constant	-20.516***	-20.516***	-20.516***	-20.565***	-20.565***	-20.566***
	(3.13)	(3.13)	(3.13)	(3.69)	(3.7)	(3.7)
R-squared	0.769	0.769	0.769	0.765	0.765	0.765
N	42492	42492	42492	39325	39325	39325

Notes: * $p<.10$, ** $p<.05$, *** $p<.01$. Regression standard errors, corrected for clustering at the state*year level, are in parentheses. All regressions include county and year fixed effects.

Table 3:
Regression Results for Droughts and Floods

Dependent variable: Incumbent party's presidential vote share in the state

	(1)	(2)	(3)
Drought measure	-.058 (.042)	.023 (.054)	.005 (.069)
Farm state		.708 (1.342)	
Farm state*drought measure		-.162** (.064)	
Farm share in income*drought measure			-1.349 (1.001)
Incumbent party's previous vote share	.57*** (.108)	.57*** (.108)	.523*** (.145)
Constant	17.127*** (5.412)	14.94*** (5.5)	31.851*** (8.872)
States weighted by turnout?	Y	Y	Y
R-squared	0.685	0.687	0.686
N	1316	1316	903

Notes: * $p<.10$, ** $p<.05$, *** $p<.01$. Regression standard errors, corrected for clustering at the year level, are in parentheses. States are weighted by turnout. All regressions include state and year fixed effects.